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EDITED BY

DAVID W. YANDELL, M. D.

*Prof. of the Science and Art of Surgery and Clinical Surgery, University of Louisville,*

AND

THEOPHILUS PARVIN, M. D., LL.D.

*Professor of Obstetrics and the Medical and Surgical Diseases of Women, College of Physicians and Surgeons of Indiana.*



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# THE AMERICAN PRACTITIONER.

FEBRUARY, 1877.

Certainly it is excellent discipline for an author to feel that he must say all that he has to say in the fewest possible words, or his reader is sure to skip them; and in the plainest possible words, or his reader will certainly misunderstand them. Generally, also, a downright fact may be told in a plain way; and we want downright facts at present more than any thing else.—RUSKIN.

## Original Communications.

### RESULTS OF CLINICAL STUDIES RELATING TO PHTHISIS.

BY AUSTIN FLINT, M. D.

*Professor of the Principles and Practice of Medicine and of Clinical Medicine  
in the Bellevue Hospital Medical College.*

The limitation of this article to my own clinical studies may convey an impression of egotism which I desire to disclaim. My reasons for making the results of these studies the subject of an article will, as I trust, remove such an impression. In treating of the subject, the personal pronoun *I* will appear oftener than good taste would dictate, could its frequent use be avoided. Let me say, without further remark by way of apology, that, in behalf of the studies, the results of which will be considered in this article, I have no wish to claim any credit beyond that which may belong to patience, industry, good intentions, and an honest endeavor after truth.

In the early part of the year 1870, I resolved to study analytically the records of cases of phthisis which I had made during a period of about thirty-five years. The number of

cases was considerable, amounting to six hundred and seventy. The first step was to prepare an abstract of each of the cases, embracing all the important facts which had been noted. This was a work of no small magnitude, requiring all the leisure which I could devote to it for about a year. Then, selecting from each of the abstracts the facts relating to different points of inquiry, collating these facts, and deducing therefrom conclusions based on the results of the analytical study, required not a little drudgery and long-continued work. Devoting to this task the time which I was able to appropriate to it, with frequent interruptions, the undertaking was finished in a little over five years from its commencement, and a volume entitled "Phthisis: its morbid anatomy, etiology, symptomatic events and complications, fatality and prognosis, treatment and physical diagnosis, in a series of clinical studies," was published in the autumn of 1865.

The object of this article is to consider some of the results of these clinical studies; and the question may arise, wherefore are you called upon to do this; why not be satisfied to let the volume speak for itself as regards the results? In answer to this question, I will state the circumstance which led me to think of writing the article. An editor of a medical journal and a personal friend, in a brief notice of the volume, after speaking of the author in complimentary terms, concludes as follows: "We must confess that our feeling, after reading the book, was of regret that Dr. Flint should have sacrificed so much labor for such unsatisfactory results. It is easy to appreciate the patience and the time which must have been consumed in comparing and analyzing so large a number of cases. And yet, as the result of all this time, labor, and patience, what do we find? hardly a single new fact: while, then, we must thank Dr. Flint for adding to our collection of facts on the subject of phthisis, we must regret that so much good work should be of so little value."

This quotation is not made under any sense of wounded vanity or injured feelings. It was written doubtless with sincerity, and in a spirit of kindness. I make the quotation, in



the first place, regretting its bearing on the method of study pursued. As it seems to me, our knowledge of diseases, in regard to causation, clinical history, pathological laws, diagnosis and management, can have no other solid foundation than the results of analyses of recorded cases. I say this without entering here into a discussion of the merits of the method. But, holding the opinion stated, I should be sorry to have my labors in that direction furnish an occasion for discouraging others from pursuing this method of study. The recording of cases and the analyses require much time and patience, without the ability to foresee the value of the results. But the results can never be valueless. It may be that a long and tedious analytical study of a large collection of recorded cases fails in leading to any new or striking developments. This negative result, however, is of importance, confirming, as it does, facts already recognized as such, and affording protection for them against the assaults of theory or speculation. One who engages in this method of study must incur the risk of not evolving thereby any great truths; but, aside from the value of a negative result, there is a compensation for the labor in the personal benefit derived from the study.

I have made the quotation, in the second place, and chiefly as introductory to the question, what are some of the results of my clinical studies relating to phthisis? I proceed at once to the answer to this question.

At the time when I entered upon the study of my cases (1870), certain views had recently been introduced in this country, which were subversive of those generally held. They were embraced in the admirable work by Niemeyer, a translation of which appeared in 1869. These new views—mostly of histological parentage—were at once accepted by many. Among these views were the following: The small, semi-transparent, hard bodies which, existing in great numbers, and often in other organs as well as in the lungs, are characteristic of acute miliary tuberculosis, were considered (after Virchow) as the only morbid products entitled to be

called tubercles. When associated with the intra-vesicular exudation known as infiltrated tubercle, they are either secondary to the latter, or dependent on an auto-infection from the degeneration and absorption of morbid exudations elsewhere than in the lungs. It is the secondary production of these bodies which constitutes the chief danger in chronic phthisis. With reference to the latter doctrine, Niemeyer declared, "We have no hesitation in saying that the greatest danger for the majority of consumptives is, *that they are apt to become tuberculous*;" that is, the greatest danger is from the secondary production of the granulations to which the name tubercles was restricted.

Now, passing by the question as to the propriety of the limitation of the terms tubercle and tuberculosis, whether the so-called true tubercles are, in the manner just stated, secondary, and whether the chief source of danger in phthisis is the liability to their becoming developed secondarily, are questions to be settled by the post mortem appearances in a sufficient number of cases. The establishment of this doctrine of auto-infection requires that tubercles, when present, should be constantly associated with degenerated morbid products in the lungs or elsewhere; that there should be good grounds for considering the latter of older date than the former, when they are associated, but without great disparity in age; and that the number of tubercles should be, in some measure at least, proportionate to the amount of antecedent degenerated products. The burden of proof belongs to the advocates of the doctrine. To assume the doctrine to be true on theoretical grounds, and assert that the source of an auto-infection is apt to be overlooked, or that it may have existed and disappeared before death, is equivalent to begging the question. The records of my recorded cases were not sufficiently full to furnish data for a positive conclusion respecting this doctrine. It is a doctrine to be proved or disproved by future studies. At present, it rests on a conjecture which, probably, had its origin in the facts developed by Villemin and others in relation to the causation of morbid products, either analogous to,

or identical with, tuberculous granulations in certain animals, especially the rabbit, by inoculation with septic matter.

With reference to the doctrine that the danger in chronic phthisis is chiefly from tuberculous granulations, or miliary tubercles, my studies furnish data for a positive conclusion. Of sixty-three fatal cases, excluding cases of acute tuberculosis and those in which pneumo-hydrothorax from perforation occurred, in thirty-three neither the presence nor the absence of granulations, or miliary tubercles, was noted; and it is a fair inference that they were either absent or not present in great abundance. Of the remaining thirty cases, in four the absence of granulations and miliary tubercles was noted; in ten they coëxisted with great destruction of the pulmonary structure, as denoted by solidification, liquefied morbid products or cavities, the number of granulations, or miliary tubercles, being sufficient to render them of more or less importance in five; in five cases the lung on one side contained granulations, or miliary tubercles, in more or less abundance, the other lung presenting exudation, liquefied products or cavities, the number of granulations rendering them of considerable importance in two; in eleven cases granulations or miliary tubercles were present without great injury from associated morbid conditions, and of these eleven cases they were of importance in determining the fatal result in seven, death being attributable to grave complications in four.

These results warrant the conclusion that in the great majority of the cases of chronic phthisis, the danger is either from what, according to Niemeyer and others, constitutes the phthisical affection, namely, either the exudation into the air-cells, together with the subsequent processes involving damage of the pulmonary structure, or to grave complications, such as pleurisy with effusion, intestinal ulcers, peritonitis, and not to the development of granulations or miliary tubercles; that is, in other words, to the supervention of acute tuberculosis. The latter is an infrequent cause of death in cases of chronic phthisis.

Certain of the new views of phthisis relate to its etiology. It is stated in Niemeyer's work that tubercles are developed from the absorption of the degenerated morbid products in acute pneumonia, pleurisy and pericarditis; and that, as a rule, phthisis is preceded by chronic bronchial catarrh or bronchitis. With regard to the latter statement, Niemeyer used these words, "It (*i. e.* phthisis) arises with rare exceptions through extension of a chronic catarrh." "A simple genuine catarrh may extend into the air vesicles in a person of apparently perfect health and vigor. Healthy men should never feel sure that they will not die of an acute or a chronic catarrhal pneumonia—that is, from phthisis—proceeding from a cold and resulting in caseous infiltration and destruction of the pulmonary substance." He proposed as the most appropriate name for pulmonary phthisis, "chronic catarrhal pneumonia."

These views were subversive of those based on the clinical researches of Louis, published just fifty years ago. Yet the new views were at once adopted by not a few, notwithstanding their tendency was to change very materially the treatment of patients affected with phthisis. Niemeyer stated explicitly that he proposed the name catarrhal pneumonia with special reference to its influence on prophylaxis and therapeusis.

It is easy to perceive the reasoning which led to these views, but manifestly they are to be either proved or disproved, not by argument and discussion, but by an appeal to clinical facts. Their correctness or incorrectness hinges plainly on these questions: Does the analysis of a sufficiently large number of recorded cases of phthisis show that this disease follows acute pneumonia, pleurisy and pericarditis, often enough to denote a causative relation; and does such an analysis show that a chronic catarrh or bronchitis commonly precedes the development of the disease? *Per contra*, do analyses of cases of acute pneumonia, pleurisy, pericarditis and chronic bronchitis, show that phthisis is developed as a sequel in a greater or less number of instances? The results of my analytical studies have a positive bearing on these important questions.

The occurrence of acute pneumonia as an antecedent affection was noted in only nine cases; and of these nine cases, in only six did phthisis follow immediately or quickly, the intervals, in the remaining four cases, varying from six months to four years. Of one hundred and three cases of acute pneumonia which I analyzed in 1861, in not a single case was this disease followed immediately or quickly by phthisis.

The occurrence of pleurisy, as an antecedent affection, was noted in twenty-two cases. In eleven of these cases the pleurisy preceded the phthisis after intervals varying from four years to several months. In eleven cases the phthisis followed the pleurisy either immediately or quickly. In 1852 I analyzed forty-seven cases of pleurisy which I had recorded, and, of these cases, in only one instance was the occurrence of phthisis as a sequel certain.

Of bronchitis as an antecedent affection, its absence being determined by facts relating to cough and expectoration, the histories of one hundred and twelve cases contained information. Of these cases, in seventy-two the cough, for a period varying from a few weeks to several months, was dry. A primary bronchitis, it is assumed, may be excluded by this fact. In seventy-four cases, the cough was for either weeks or months slight. In only one case was it noted that expectoration coincided with the commencement of cough. I have not analyzed a collection of cases of chronic bronchitis; but is it not a matter of common observation that, while this affection may lead to asthma and emphysema, and is liable to persist for years, phthisis, as a sequel, is one of the rarest of rare events in clinical experience?

It does not seem to me to be assuming too much in claiming for the results of my clinical studies, in relation to an etiological connection between acute pneumonia, pleurisy and chronic bronchitis, that these affections have little or no tendency to the development of phthisis, especially when it is considered that these results are in strict accordance with those obtained by Louis half a century ago. Should the statement of any author, no matter how great may be our

respect for his opinion, have the weight of a feather in the balance against overwhelming clinical facts? In view of the important practical bearings of correct views respecting a causative relation between these diseases and phthisis, I will venture to ask, if the results of my studies are not of positive value in this connection?

Other of the new views had reference to bronchial hemorrhage in its relations to phthisis. Niemeyer taught that bronchial hemorrhage occurs more frequently than has been generally supposed, when the lungs are free from phthisical disease; that, under these circumstances, the occurrence of hemorrhage implies a predisposition to phthisis, although there be no direct connection between the hemorrhage and phthisis; that phthisis, however, in a certain proportion of cases is caused by blood remaining in the bronchial tubes, and that hemorrhages occurring in persons affected with phthisis are likely to hasten a fatal termination.

That bronchial hemorrhage does occur oftener than is generally believed without the evidence of phthisical disease, the results of my clinical studies prove. Louis deduced from his researches the conclusion that a hemoptysis not following an injury of the chest, and not vicarious, as in some cases of suppression of the menses, renders the existence of phthisis "infinitely probable." He based this conclusion on the result of the inquiry made to all patients under his observation for nearly three years, who were affected with other diseases than phthisis, whether they had ever expectorated blood. The answer was negative in every instance, excepting when the chest had received an injury, or the menses had been suddenly suppressed. Louis, however, admitted that well attested facts appeared to show some happy exceptions to the rule. How many patients were questioned is not stated, but it is certain that had the number been larger, instances of bronchial hemorrhage without other evidence of phthisis would have been found. In connection with this point of inquiry, I have introduced abstracts of the histories of nine cases in which repeated

attacks of bronchial hemorrhage had occurred within periods of time varying from one year to twenty-three years, without the evidence afforded by other symptoms, or by physical signs of the existence of phthisis. In addition to these cases, I have reported one case in which death took place two years after a hemorrhage, disease of the liver being the cause of death, and the autopsy showed no trace of phthisis. I have added an account of five cases, coming within my knowledge, but not under my observation, in four of which the patients were living and well after intervals from the dates of the hemorrhage, varying from eleven to forty years; and in the single remaining case, the patient died of cancer of the liver fourteen years after the hemorrhage. Moreover, in thirty-seven of the histories of my cases of phthisis, it was noted that hemoptysis occurred once or repeatedly, when cough and other evidence of phthisis followed after an interval of greater or less length. In nine of these cases, the intervals varied from two to sixteen years.

These results show the correctness of Niemeyer's statement with regard to the occurrence of bronchial hemorrhage when phthisis does not exist. The analysis of three hundred and eighty-six cases of hemoptysis by John Ware—the cases having been noted in private practice during a period of about forty years—also sustains this statement. In sixty-two of Ware's cases, the patients were afterward known either to be living in ordinary health, or to have died of other diseases having no connection with phthisis, the length of time during which this immunity continued varying from two to thirty-seven years. It is probable, however, that some of these cases were instances of recovery from phthisis.

The results of my studies show also that Niemeyer was right in saying that bronchial hemorrhage implies a predisposition to phthisis. Cases of hemoptysis occurring without coëxisting phthisis, and not followed by this disease, are rare; whereas of sixty-three cases in which hemoptysis preceded phthisis, this disease followed either quickly or at periods more or less distant from the date of the hemorrhage in thirty-



seven. It is needless to say that this fact is one of great practical importance. While the occurrence of bronchial hemorrhage is not proof that phthisis actually exists, nor that it will ever be developed, in the great majority of cases, if the disease be not present, it will sooner or later follow. The instances of exemption are exceptions to the rule.

That phthisis may be produced by the local effect of coagulated blood within the bronchial tubes, is a conjecture but little, if at all, sustained by clinical facts. I have given a report of seven cases in which a profuse hemorrhage was coincident with the occurrence of grave local and general symptoms, death taking place within a short period. These cases are rare, but the fact of their occurrence is important. That the grave symptoms are due to the effects of coagulated blood within the air-tubes, seems less rational than that the hemorrhage is incident to a sudden and considerable increase of phthysical conditions. A single case which I have introduced, in which coagulated fibrin in the form of casts of the bronchial tubes, was expectorated in great abundance after a profuse hemorrhage, goes to show that this blood-constituent, as well as some foreign bodies, within the bronchial tubes, may be well tolerated.

Finally, Niemeyer did not consider bronchial hemorrhage as ever being of favorable omen in cases of phthisis. On the contrary, he stated "that the bronchial hemorrhages which occur in an established case of consumption cause chronic pneumonia and destruction of the tissue, and thus hasten the fatal termination." The inference is that hemoptysis in general must be regarded as an unfavorable event in cases of phthisis. Ware's statistics, already referred to, which were published in 1860, proved the incorrectness of this view. They showed that hemorrhages, even when repeated and profuse, occurred oftener in cases of recovery and of prolonged duration, than in those which were fatal and which ended in a comparatively brief period. This was a striking and important result of his analytical study of hemoptysis, rendering his paper on this subject one of the most valuable of the contri-

butions to clinical medicine within the last twenty years. My studies led to the same result. Comparing, as regards fatality or recovery, arrest and tolerance of the disease, ninety-eight cases in which hemoptysis occurred, with eighty-one cases in which this event was wanting, the former showed the larger number of recoveries, and notably more instances of arrest and tolerance, than the latter. It may then be laid down as a well established fact that, with some exceptions, bronchial hemorrhage is of favorable significance in cases of phthisis. Assuming this statement to be correct, I ask if clinical studies relating to this point of inquiry have not been of some value?

I pass by studies relating to pharyngitis, laryngitis, pneumonia, pleurisy and effusion, perforation of lung, perineal fistula, pregnancy, together with other symptomatic events and complications, in order to devote the remainder of this article to the results of the analytical study of cases ending in recovery; of cases of arrested or non-progressive phthisis; of cases of slowly progressing phthisis; of fatal cases with reference to circumstances affecting duration and causing death, and some points of inquiry relating to the management.

Of the cases of phthisis which were noted during thirty-five years, in forty-four recovery took place. The proof of recovery was the regaining conditions of health which existed prior to the development of the disease, pertaining to weight, muscular strength and general vigor, together with the disappearance of all pulmonary symptoms; this restoration being maintained for a series of months at least, and physical exploration of the chest disclosing no signs of any morbid process in the lungs. In order to forestall distrust respecting the accuracy of the diagnosis in any of these cases, I have given a synopsis of the histories of all these cases, embracing the diagnostic symptoms and the physical signs. The aggregation of forty-four cases of recovery from phthisis, is a noteworthy fact. It would be of interest in the study of these cases to institute comparisons with similar collections made by others; but I am not aware that such a collection is to be found in medical literature.

The analysis of a considerable number of cases of recovery from phthisis can not fail to lead to important conclusions. It is fair to conclude that circumstances which are common to all, or a large majority of these cases, have had more or less agency in the recovery; and especially is this conclusion a fair one, if these circumstances are characteristic of cases ending in recovery, that is, if they are wanting or less frequently present in fatal cases. I refer now to circumstances irrespective of the treatment. Negative as well as positive results of the analysis are important, inasmuch as they show what circumstances do not affect the recovery.

Considering, in the present connection, the results as involving elements in prognosis, they warrant the following conclusions: Other things being equal, the prospect of recovery is in proportion as the phthisical affection is small. A considerable amount of exudation, and even a large amount together with the existence of cavities, however, do not preclude recovery. The prospect of recovery is not greater after, than before, thirty years of age. In twenty-six of thirty-six cases, the ages were under thirty, the earliest age being nineteen years. Sex appears to have no influence on recovery. The absence of family predisposition to the disease, is not of much importance as regards a favorable prognosis. Hemoptysis, although repeated and profuse, is not unfavorable. Chronic laryngitis, pleurisy with effusion, as antecedent and intercurrent affections, and perineal fistula, are not unfavorable. Finally, in most of the cases the conditions of health, irrespective of the pulmonary affection, were not greatly impaired. These conditions were represented by symptoms pertaining to the digestive system, nutrition, muscular strength, circulation, temperature of the body, etc. Moreover, most of the patients who recover from phthisis are persons of resolution and perseverance; persons who appreciate the nature of the disease, and are determined to overcome it, bringing the power of the will to bear upon means for recovery.

My studies show that in a little over one-seventh of the cases ending in recovery the disease recurs, the intervals in

six cases in which recurrence was noted varying from one and a half to six years. In one case the disease recurred twice, and the patient is now living and free from pulmonary disease. In all the cases in which a single recurrence took place, the recurrent affection proved fatal, showing that the fact of recurrence renders the prognosis extremely unfavorable.

May I not claim for these conclusions that they have not only interest, but a certain measure of importance?

In some cases of phthisis there is an arrest of the disease; it ceases to be progressive, and retrogression, more or less, ensues, but recovery does not take place. Among my cases were thirty-one belonging in this category. It seemed a rational supposition that for analytical study with reference to elements in prognosis, this group of cases is hardly inferior to the collection of cases ending in recovery. In each of the two groups there was an arrest of the disease; in one the recovery being complete, and in the other there being only an approximation more or less close to recovery. I analyzed, therefore, these cases of arrest without recovery, precisely in the same way as I had analyzed the cases of recovery. The results of the analysis, in respect of age, sex, family predisposition, hemoptysis, and the conditions of health, irrespective of the pulmonary affection, were essentially the same as the results of the analysis of the cases of recovery. This uniformity in the results of the two analyses, tended to confirm their correctness. The point of difference in the two analyses related to the amount of pulmonary disease. In sixteen of twenty-six cases of arrest without recovery, the amount of disease was considerable, cavities existing in five of the sixteen cases. Thus a considerable amount of disease, while it does not preclude arrest, is unfavorable as regards the prospect of recovery.

In all of the non-progressing cases, several months, and in the majority many years, elapsed without any progress of the disease.

Another class of cases is characterized by notable slowness of the progress of the disease. Of my cases, ten belonged

in this group, the duration in six cases ranging from eight to forty years, and of the remaining four cases three having been lost sight of, and in one the patient still living. An analysis of these ten cases gave essentially the same results as regards age, sex, family predisposition, hemoptysis, and the conditions of health, irrespective of the pulmonary affection, as the analyses of the cases ending in recovery and the cases of arrest without recovery. Hence, it is a fair conclusion that the circumstances which are consistent with arrest, either with or without recovery, are alike consistent with slowness of the progress of the disease, or the tolerance of it. In not one of these ten cases was the phthisical affection noted as small. The results of the analysis of these cases were thus corroborative of the results of the analysis of the cases of recovery and of the cases of arrest without recovery.

As bearing on the elements in prognosis in respect of the probable termination and the duration of the disease, the analytical study of fatal cases is important. What are the complications, events, or circumstances, which cause death and shorten the duration of phthisis? With reference to this question, I selected from the cases in which the duration was determinable, two groups. In one group were embraced the cases in which death took place after periods varying from three to forty years; and, of the cases in this group, I analyzed separately the single case of forty years' duration, one case of thirty-one years' duration, one case of twenty years' duration, seven cases in which the duration was between ten and fifteen years, five cases in which it was between five and ten years, and nine cases in which it was between three and five years; the whole number of cases in this group being twenty-four. In the second group were embraced cases in which the duration was not over a year, cases under and over six months being analyzed separately. Without citing the results of the analyses and comparisons in the study of these several divisions and subdivisions of cases, the following are conclusions deduced therefrom: The duration in fatal cases is shorter in women than in men; sex, therefore, has some

influence. Contrary to what might have been anticipated, occupations appear to have had no influence in either prolonging or shortening the duration. The duration is shorter in patients who become phthisical after, than before, the age of thirty years; a conclusion which is the reverse of what might have been expected. The intemperate use of alcoholics does not seem to shorten the duration. In a certain proportion of cases death, after a short duration, is caused by important complications, namely, perforation of lung, pleurisy with effusion, intercurrent pneumonia, peritonitis, meningitis, and intestinal disease. In a few cases death follows pathological conditions which are either coincident with, or immediately consecutive to, profuse bronchial hemorrhage. In some cases death is referable to the amount of the pulmonary affection and the rapidity of the phthisical processes; these are cases of *phthisis florida*, or "galloping consumption." In rare instances death, after a short duration, is attributable to the rapid production of miliary tubercles; in other words, to the supervention of acute tuberculosis. The disease is apt to end fatally, after a short duration, when developed during pregnancy. Hyperpyrexia, frequency of the heart's action, and defective alimentation from notable impairment of appetite and digestion, are circumstances significant of a fatal result and shortness of the duration of the disease.

Entering upon studies relating to the treatment of cases of phthisis, the first object of study which suggested itself was the analysis, separately, of the cases of recovery, the cases of arrest without recovery, and the cases of slowly progressing disease, with reference to remedies and hygienic measures employed in these groups of cases. The analyses led to results involving a fact in clinical medicine, which, if not new, is certainly not fully recognized. It is that phthisis, in a certain proportion of cases, tends intrinsically to recovery; in other words, that this disease undergoes arrest or ceases to progress from its own limitations. Is this fact in accordance with views generally held by medical writers and practition-

ers? Is it not at variance with these views? It is the common belief, if I mistake not, that the tendency of phthisis is to be progressive. If it cease to progress, and become retrogressive, ending sometimes in recovery, its favorable course is imputed to measures of management, or to some extrinsic circumstances. So strong is the belief in the absence of an intrinsic tendency to arrest and recovery, that a candid physician is apt to distrust the accuracy of his diagnosis when a patient whom he had considered phthisical recovers; and cases of recovery from phthisis which are related, are generally regarded as of doubtful authenticity. Do I err in saying that, in the judgment of most medical men, an error in the diagnosis of phthisis is more probable than recovery from this disease? I proceed to state the results of my studies as bearing on this important point of inquiry.

Out of forty-four cases ending in recovery, in eight not only was there no medicinal treatment of importance, but no material change in the habits of life; in other words, there was no hygienic treatment, the recovery taking place without any appreciable influence brought to bear upon the disease, and therefore purely from an intrinsic tendency. Five of the patients were men, and three women. Of the female patients, one was a school teacher, her duties confining her quite closely within doors. The other two female patients were sisters; both parents had been tuberculous, and a sister and two brothers have died with phthisis. These two sisters are all who remain of the family. They recovered without any material change in habits of life; whereas no efforts had been spared to save the lives of their sister and brothers, traveling, change of climate, together with remedies, being resorted to in vain, although probably with the effect of retarding the progress of the disease. Of the five male patients, one was a farmer, one was a constable, two were practitioners of medicine, and one was a clerk in a clothing store. All were temperate as regards the use of alcoholics; all were under thirty years of age; in all the phthisical affection was small or moderate.



Of the thirty-one cases of arrested or non-progressive phthisis, in six there was no medicinal treatment of importance, nor any material change in the habits of life. Two of these patients were women and four men. Of these six cases, one is known to be living and in good health; one has died since my analysis was made, and the present condition of health in the remaining cases is unknown. The length of time noted from the development of the disease up to the last record in the cases, severally, varied from seven to twenty-seven years.

Adding together the cases of arrest with, and those without, complete recovery, the number is fourteen. Now it is of interest to compare these cases with those among fatal cases in which there was no medicinal treatment of importance, and in which no important hygienic measures were employed. Among the fatal cases in my collection there were sixteen in this category; thirteen of these patients were men, and three were women; the ages varied between eighteen and fifty-two years. Three of the patients were day-laborers; in seven cases were represented seven different occupations, namely, fuller, joiner, clerk, planter, lawyer, teacher of chemistry and coppersmith; and in six cases the occupations were not noted. In one case the duration of the disease was twelve or fourteen years; and in another case it was ten years. In seven cases, the duration was from seven or eight months to three years, "several years" having been noted as the duration in one case; the duration in seven cases not being determinable.

If we add to the sixteen fatal cases in which there was no treatment, medicinal or hygienic, the eight cases of recovery, the six cases in which the disease was non-progressive, and one case of slowly progressive phthisis, all belonging in the same category, the total is thirty-one cases. In these thirty-one cases the disease was allowed to go on without any active interference. In so far as we may be authorized to deduce from these cases conclusions, they are that nearly twenty per cent. of cases of phthisis, in which the disease is left to itself, will end in recovery; in a little over nineteen per cent. the disease will be non-progressive for an indefinite period; and in

a fraction over fifty-one per cent. the disease will end fatally after a duration ranging between six months and twelve or fourteen years. These conclusions would be immensely important as the basis for estimating the influence of treatment, were it proper to accept them as representing correctly the relative proportion of cases in which this disease tends intrinsically either to recovery, to become non-progressive, or to end fatally. Were they to be thus accepted, they would tend to discourage efforts in the way of treatment. The cases, however, are too few to justify conclusions for a general application. The number is, perhaps, larger than any one observer has hitherto recorded and analyzed, and, as a contribution to the study of the natural history of phthisis with reference to its intrinsic tendencies, as it seems to me, they have not a little value; but a larger collection is a desideratum. My analyses, however, suffice to show an intrinsic tendency to recovery, arrest and slowness of progress in a certain proportion of the cases of phthisis. Here, then, is a factor of unknown power for which allowance is to be made in estimating the apparent influence of remedies and hygienic measures of treatment. Is not the establishment of this fact, on clinical data, an adequate compensation for a considerable amount of labor?

To what extent do hygienic measures contribute to recovery, non-progression and slowness of progress, in cases of phthisis? It would be superfluous to say anything respecting the importance of this question; and there is no doubt as to the answer which would be given by nearly all practitioners of medicine. With a view to clinical evidence, I examined the histories of the cases ending in recovery, the cases of arrest without recovery, and the cases of slowly progressive phthisis, selecting from these three groups the cases in which the treatment was hygienic without medication. Of the forty-four cases ending in recovery, fifteen were in this category. Of these fifteen cases in all, with perhaps a single exception, the hygienic measures consisted of change of habits as regards out-of-door life. The extent of this change varied much

in different cases; but in most of the cases it was either considerable or great, involving traveling abroad, sea voyages, occupation much of the time in the open air, and a permanent removal to another climate. To what extent an influence is to be ascribed to these measures in effecting recovery, can not be determined with positiveness in view of our inability to appreciate and measure an intrinsic tendency thereto. We may rationally attribute to the measures a certain amount of influence, for it is by no means probable that all these fifteen cases would have ended in recovery purely from an intrinsic tendency.

Of thirty-two cases of arrested or non-progressive phthisis, in ten the treatment was hygienic without any special or important medication. The same remarks will apply to these ten cases, as to the fifteen cases ending in recovery.

In seven of the ten cases of slowly progressing phthisis, there was no medicinal treatment of importance; but hygienic measures—either traveling, sea voyages, change of climate or more out-of-door life—entered, to a greater or less extent, into the treatment.

Thus, in thirty-two of eighty-six cases of either recovery, arrest and non-progression, or slowness of progress, the treatment was essentially hygienic.

Of the fatal cases, in twenty-three the treatment was hygienic without any important medication. Of these twenty-three cases, the duration was determinable in fourteen. With a view to evidence of the favorable influence of hygienic measures, these fourteen cases were compared, as regards duration, with eight cases in which neither hygienic nor medicinal treatment was employed, the duration being determinable in these eight cases. This comparison showed the duration to have been not far from twice as great in the cases having hygienic treatment, as in the cases receiving neither hygienic nor medicinal treatment. The number of cases compared was small, namely, fourteen to eight; but so far as the comparison of these cases warrants any conclusion, it is that, in fatal cases of phthisis, the disease is prolonged by hygienic

treatment. Moreover, the favorable influence of hygienic treatment was shown by a comparison of these two groups of cases in another point of view, namely, with reference to notable improvement and tolerance of the disease following the hygienic measures—living in the open air, sea voyages, and change of climate.

What are the relative merits of different hygienic measures, as regards a favorable influence in cases of phthisis? To determine with precision the influence of any particular measure by the analytical study of cases, is a most difficult undertaking. Let the problem, for example, be to determine, by clinical analyses, the favorable agency of a particular climate, the requirements for this problem are a sufficiently large number of cases on each of which the climatic influence has been brought to bear for an uniform period, uniformity in all the cases in respect of the amount of the phthisical affection, the stage of the disease, the general symptoms with numerous other circumstances, and a comparison of the results of the analysis of these cases in regard to recovery, non-progression of the disease and slowness of its progress, with other collections equally large of cases observed in other climates, the latter conforming to the former cases in all regards inclusive of medicinal measures of treatment. No single observer, however large may be the number of cases which he has recorded, the records extending over ever so long a period, can expect to meet fully these requirements. Hence, all that any one can do, with whatever clinical material he has collected, is to obtain results which are necessarily imperfect as a basis for some rational conclusions. Inadequate, however, as these results may be to solve the problem with anything like mathematical accuracy, they are, nevertheless, far more reliable than general impressions formed from the recollection of a limited number of unrecorded cases.

I have endeavored to study my cases so as to reach all the results to be obtained by analysis and comparison, as the basis of rational conclusions; and I have placed before the reader the facts, relating to this point of inquiry, which are contained in the histories of the cases severally.

The hygienic measures which may be supposed to have exerted more or less influence are numerous and varied. The following classification of the different measures was made:

First. Temporary change of climate. This entered into the treatment in seventy-four cases; the whole number of cases in which the different classes of measures were employed being one hundred and fifty nine. The cases in the first class, and those in the other classes, were analyzed with reference to the number of instances of recovery, of non-progression, of slowness of progress, the number of fatal cases, the instances of improvement in the fatal cases, and the duration of the disease. After the analysis with reference to these points, the cases were studied with reference to the influence of particular climates.

Second. Change of habits from those more or less sedentary and confining within doors, to those involving out-of-door life and activity, constituted the second class of hygienic measures. Measures belonging to this class entered into the treatment of forty-four cases.

Third. The third class embraced cases in which there was change of residence from the city to the country, or to a different climate. The number of cases in this class was thirteen.

Fourth. Change of occupation distinguished the fourth class, and in this class were eight cases.

Fifth. In the fifth class were embraced the cases in which a long sea voyage, or a series of voyages, entered into the treatment, and twenty cases came under this heading.

The details of the study of the cases in these five classes can not, of course, be reproduced in this article; and I shall content myself with quoting the conclusions as embodied in the following propositions:

First. Benefit, more or less marked, is derived in a large proportion of cases of chronic phthisis, from a temporary change of climate. This benefit seems to relate more to circumstances which are accessory or incidental to the change, than to any special climatic agency.

Second. Change of habits, from those which are sedentary

and confining within doors, to those involving out-of-door life and activity, has a favorable influence in cases of phthisis, which is, perhaps, greater than that proceeding from any other class of hygienic measures. It is probable that the influence from this source explains, in part, the benefit derived from change of climate.

Third. The benefit derived from change of occupation is due to a change of habits, involving more out-of-door life and activity.

Fourth. A permanent change of residence is beneficial in certain cases, the favorable influence, being more or less attributable to accessory circumstances.

Fifth. Sea voyages have, in a large proportion of cases, a favorable influence, which is sometimes very great; and this is an accessory circumstance which, in certain cases, contributes a share of the benefit derived from a change of climate.

These conclusions may seem very moderate and tame to one who thinks that brilliant developments only can compensate for clinical work. The conclusions, however, are not without important practical bearings. Their general tendency is to lessen confidence in a purely climatic influence on phthisis, and to enhance the value of hygienic measures which are more available, involving less hardship than the pilgrimages made by so many phthisical patients, with the risk of dying away from home and friends.

Craving indulgence for the length to which this article has reached, I shall state in a few words the results of my studies relating to the medicinal treatment of phthisis. To establish the curative effects of different remedies, is certainly not less difficult than to determine the influence of particular hygienic measures. The same requirements are necessary, and we must be satisfied with rational conclusions which, in view of the results of clinical study, are to be considered as probably correct.

An analysis of eighty-four cases into the treatment of which cod-liver oil entered, led to the conclusion that this remedy was useful, although not developing proof that it exerts a

potentially curative influence. Of the curative power of the hypophosphites my cases do not afford much evidence, but the analysis was limited to sixteen cases. In seven of fifteen cases in which alcoholics were taken freely, they appeared to be notably useful. Palliative remedies, of course, constitute an important part of the treatment in cases of phthisis, but I did not undertake to analyze the histories with regard to the efficiency of these.

In conclusion, to my apology at the outset for the anomaly of an author becoming the reviewer of his own work, it may be added, it was not expected that a work, the greater part of which is devoted to dry details, would be perused by many; and it is certainly a natural, if not a reasonable desire, that an endeavor to utilize the records made during three and a half decades of life, by means of studies which extended over five years, should not be considered entirely fruitless as regards important results.

NEW YORK CITY.

## CONSIDERATIONS IN RELATION TO DISEASES OF THE JOINTS.

BY DAVID PRINCE, M. D.

It is a question why a sprain of a joint disappears in a few days in one person in one condition; and in another person, or in another condition, the acute affection sets in operation a train of diseased action which continues for years. What is the condition upon which this difference depends? It is difficult to find an explanation, except in a general or constitutional state, in the "*vis medicatrix nature*."

The general condition adequate to make the difference in the course of a local disease, tending in one case to speedy recovery, and in another to protracted morbid changes, may be inherited and permanent, acquired and permanent, or ac-



quired and temporary. The treatment of local diseases in the latter condition affords the most brilliant results from remedies directed to the temporary general condition, without which no local treatment can be of any avail. Knowledge in this direction is especially requisite in the treatment of chronic joint diseases. If a patient should be attacked by boils, ripening like cotton one after another, and coming in different localities, he would be a silly man who should think to cure the disease by local remedies. So, if bone and joint diseases arise one after another in different parts, a fact familiar to every observer, he would be equally silly who should fail to see nothing but local causes in the manifestations distant from each other.

In disease of hard bone, attended by the arrest of its circulation to the extent of necrosis, the resulting sequestrum becomes a dead or foreign body, keeping up a diseased action which is independent of a general dyscrasia; and in this condition, the removal of the foreign body leads to a speedy cure. If, however, the general condition favorable to the local disease remains, there may be a continuance of local disease, and the surgeon may wonder at his disappointment.

A necrosis of the spongy bones occurs less frequently, because there is room for the expansion of the more numerous vessels, and nutrition is less dependent upon the superficial periosteal supply. It is on this account that the disease more often results in chronic inflammation than in necrosis.

The classification of joint diseases has not been improved since Brodie wrote his book on "Diseases of the Joints."

First. Diseases commencing in the synovial membrane, resulting in swelling, effusion, or ulceration; running a simple course, or extending to adjacent parts. The very convenient word *synovitis* had not been introduced in Brodie's time.

Second. Diseases commencing in the ligamentous and cartilaginous structures, painless from the absence of nerves of sensation, but attended with sympathetic pain and spasm. From the absence of pain, and the hidden position of the diseased tissues, very considerable destruction of the over-

lying synovial membrane, and the extension of the disease to the underlying bone, exposing its nerves, which acquire the sensitiveness characteristic of osseous inflammation, must occur before the case is taken to be serious. No convenient *itis* has been invented for this.

Third. Diseases commencing in the bone, resulting in gradual swelling; the formation of an abscess, or of hyper-nutrition generally invading the joint, but sometimes not. The difference in symptoms between rapid and slow progress is as great here as in other parts. The slow cases of "white swelling" result in a shape of joint reminding one of dumb-bells placed together, with big extremities and attenuated shanks. This disease may remain confined to its original seat in the bone, or extend to the joint, with ulcerative or suppurative inflammation, or the exudation of gelatinous synovia, which has no great tendency to suppurative metamorphosis; elastic or fluctuating and tempting to the bistoury, but non-responsive to its introduction.

Several diseases, or kinds of disease, may be included in each of the three divisions of the classification. The classification itself is chiefly useful in aiding the conception of the subject. The diseases so run into each other in their progress, that it may not be easy or important to determine the seat of the disease in its beginning. The special mechanical indication, to avoid the contact and friction of adjacent parts, is the same in all. This is an appreciation which has been attained since the time of Brodie, and which has done more to lessen the severity and shorten the duration of joint diseases, than all other things put together.

The brilliancy of this innovation in the treatment of diseases of joints—for the practical realization of which we are indebted to the use of adhesive plaster for extension, and to the mechanical genius of Henry G. Taylor—has diverted attention from the lesson which the older living members of the profession learned from the reading of Abernethy "On the Constitutional Origin of Local Diseases." There is a temptation in those who practice a specialty to take a view of local

diseases altogether too narrow, and surgery is not exempt from the tendency. An operation or an apparatus is expected to stop the progress of a disease, which owes its origin and continuance in great part to causes inherent in the vital condition of the general system, of which the local disease is a manifestation, or to the local condition of a distant part, of which the disease in hand is a sympathetic effect.

An instructive example of this constitutional tendency was seen by the writer last August, in the "Hospital for the Ruptured and Crippled" in New York City. The case was that of a child about eleven years old, on whom Dr. Lewis A. Sayre is said to have made the operation for excision of the head of the femur about four years ago. At the time of observation in August, 1876, several open sinuses occupied the seat of the incision; several other ostitic and subperiostitic suppurative inflammations had developed themselves since this surgical procedure. The case is the more significant because the distinguished surgeon referred to, together with Dr. Louis Bauer, formerly of Brooklyn and more recently of St. Louis, have done more than all others together in this country, to give prominence to excision as a remedy in joint diseases. They have gone farther, and attempted to depreciate the importance of the constitutional element in the natural history of the disease.

The writer has now under observation a case in which there is gelatinous exudation in both knee-joints, nearly absorbed in the left and in progress of absorption in the right, with bony enlargement of the epiphysis, showing involvement of bone in the morbid action; subperiosteal inflammation resulting in bony exfoliation above the right knee; a similar condition of the lower anterior portion of the tibia on both sides; a recovered disease of the right ankle-joint; an exfoliating necrosis of the humerus on both sides, near the upper extremities; a similar condition of the upper portion of the sternum, and of the crest of the left ilium. A year ago the tibia was flexed to an acute angle with the femur on both sides, a condition which has been gradually but completely removed by weights

and pulleys. The constitutional condition in this case was very low a year ago, at the time of the beginning of the treatment, which has been both mechanical and constitutional. At present the general health appears to have been completely restored, and the local tendencies are upward in accordance with the general condition.

The practice of excision in such cases must lead to disappointment, unless the general tendency is changed. If this change of general tendency is secured, the osseous, cartilaginous and synovial inflammations gradually subside, and the form of the joint assumes its natural shape. If suppuration, pressure, and ulcerative absorption have resulted in openings with the exterior, the condition gradually ameliorates, and with more or less deformity or restraint of movement, the joint ceases to have any active pathological condition. If minute portions of bone have become necrosed, they become detached and float away in the stream of pus. When, however, there comes to lie in a joint a distinct detached portion of spongy bone of such size as not readily to flow away with the purulent discharge, then the rule of practice applicable to hard bone applies equally to the soft. In necrosis of hard bone, it is the rule to wait until the dead has become detached from the living, so that it can be lifted away. No one thinks of chiseling off an exfoliation, or chiseling out a sequestrum, which is still in continuity with the living bone. If a necrosis of spongy bone is no surgical exception, the excision should be postponed until the continuity of the dead with the living has clearly ceased. Then it is clearly no excision, but an extraction of what has become a foreign body.

There may arise cases in which, after the constitutional condition has taken the upward tendency, the removal of the diseased extremities of the bones involved, may shorten the time of the recovery. If, however, the favorable turn has taken place with the locally diseased parts not removed, the practitioner is warranted in seeing what further can be done by his therapeutics, so that with more time he may save, instead of sacrificing, a joint. If, on the other hand, there is

no upward change in the constitutional condition which originally caused the disease, or if caused by an injury, favored its progress downward into the chronic state, the remaining portions of bone are likely to become carious or necrosed, and in such a condition the patient, like the one referred to in the "Hospital for the Ruptured and Crippled," will be no better for the proceeding, with the risk of dying soon after the operation, from some of the many accidents attending large wounds.

The rule arrived at by this presentation of the subject is not applicable to the question of making free incisions into joints which are already open, or which contain pus which has passed to the condition of putrefaction. An unimpeded discharge diminishes the local irritation, and the free openings make it possible to wash away the putrefactive material and to apply antiseptic dressings. The question whether or not the contents of a swollen joint are putrid may be safely determined by the aspirating needle, which may at the same time diminish the internal pressure by loosening the amount of the fluid. It is well, in this proceeding, to dip the needle into melted carbolic acid in order that any germs of putrefaction adhering to the needle may be destroyed, and to cover the places of puncture after the withdrawal of the needle with isinglass plaster, which, when a little dried, may be smeared with carbolized oil.

If it is found that the contents—whether pus, serum, or gelatinous exudation—are not putrid, it is not easy to see any reason for an incision, much less an excision. The fluid serves the mechanical end of keeping the articular surfaces asunder, and of thus answering one of the purposes of extension, without in any possible way being a source of local irritation. Nothing can be a softer cushion for surfaces which are rough and inflamed to a high degree of sensibility, than either of the kinds of fluid which are exuded into the cavity of a diseased joint. An amount of fluid sufficient to produce painful distension may be diminished by aspiration, but a complete evacuation would be a blunder.

Joint diseases do not differ from other local diseases in the demand for rest, relative or absolute; and whether relative or absolute, depends upon the question of the possibility of securing the relative without the absolute. When any vibration of a diseased part is immediately attended by pain, the patient will himself restrain his general movements, and resist the injudicious interference of others. When, however, the pain comes some time afterward, a patient will rarely reason out the connection between the two. If unrestrained, he will exercise as long as he likes, and fail to attribute his subsequent sufferings to the indulgence in motion.

It is here that the medical adviser has to contend against not only the inclinations of the patient and the friends, but against a medical sentiment that has been recently worked up by parties who are interested in the sale of apparatus. The mischief which follows the dependence upon a "portable splint" is irretrievable in the double sense of permanently impairing the patient's healthy form, and impairing the practitioner's reputation.

It is urged everywhere that the patient must have exercise in the open air to keep his health up; and because walking and riding in a carriage are not sufficiently painful to suppress the patient's enjoyment of the pastime, neither he nor his friends are likely to attribute the suffering of the following night to its true cause.

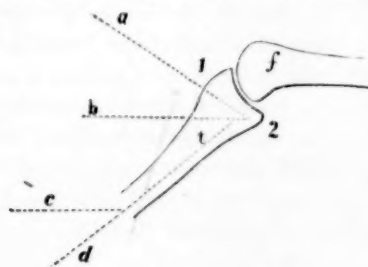
To those who know, it is evident enough that the pain, the sleeplessness, and the opiates, do vastly more mischief than the open air exercise can do good. That the patient and friends never appreciate the exemption from the terrible symptoms of protracted joint diseases which comes from the early enforcement of absolute rest, may be overbalanced in the mind of the practitioner, by his own escape from attendance upon a case in which he must appear almost helpless in the presence of the suffering patient and his anxious friends. It requires intelligence, personal force, and great watchfulness, to secure the efficient observance of enforced rest; and one who can not succeed, either from his own defect or the oppo-

sition of patient and friends, will generally come out with the best reputation by retiring from the case. In the acute stage of a joint disease, the enforcement of absolute rest is free from disaster; and, in most of the cases which are not wholly constitutional in their origin, the case will terminate "by resolution."

The wrist and elbow-joints can be surrounded by splints, so as to secure absolute local rest while the patient moves about; but the joint of the shoulder, and those of the spine and the inferior extremities, can only be partially restrained by apparatus. Lying in bed is the rational remedy. It must be remembered that the advantage of exercise in the open air is relative, and may, therefore, be injurious when inseparable from those vibrations and movements which aggravate the local disease.

The portable apparatus permitting motion is for a later period, when the case is in the stage of recovery, and when it is desired to prevent the consolidation of exudations and adhesions. For the hip, knee and ankle, it is desirable that the splint should extend below the foot, so as to receive the whole weight of the body. The latest modifications by Dr. C. F. Taylor, of New York, answer the purpose.

It is not in the plan of this paper to speak of the correction of deformities, but a hint may be given with regard to the direction of extension when a deformity exists. The extension should be so applied that one of the internodes is not converted into a lever, by which a portion of its joint surface is caused to press upon the corresponding joint surface of its



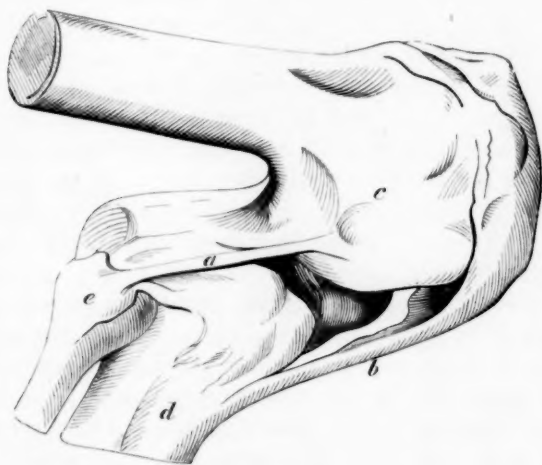
adjacent internode. The accompanying diagram illustrates the point. Let the bone *t* be flexed upon the bone *f*, and retained by the changes and products of inflammation, so that one joint surface will not readily glide upon the



other; and let it be attempted to overcome the pressure of one surface upon the other. The inspection of the diagram leads to the selection of *d* as the most advantageous direction for the relief of pressure; while the lines *a* and *b* are most advantageous for the relief of pressure and the correction of deformity. The direction *c*, as a line of extension, must increase the pressure at 1 as much as it is diminished at 2, and can only be attended with mischief.

This mechanical principle is applicable to all joints, and the neglect to observe it is the reason of most of the failures in the unsuccessful attempts to employ extension.

The accompanying cut, taken from Tamplin on Deformities, further illustrates the subject:



*a* External lateral ligament.    *b* Ligamentum patellæ.    *c* Femur.  
*d* Tibia.    *e* Fibula.

If the problem is to relieve the pressure of the articular surfaces in the progress of an inflammation of a joint, after a considerable displacement has occurred, or when there has come to be a fixedness in the flexed position, the force must

be so applied as not to increase the pressure upon any surface whatever within the joint.

With regard to the treatment of the constitutional disease or diathesis—without which diathesis we should never see a chronic inflammation, independent of some parasitic or other foreign cause of irritation, introduced from without or originated within the system—it suffices to say that after the removal or the alleviation of the irritating cause which acts upon the general system to tax its nerve power, that mode of treatment is to be pursued which is best fitted to secure an appetite for food and its digestion. Nutrition is the *sine qua non* in the treatment of exhausting chronic diseases. This end is best secured by indirection. Let it be assumed that some obstruction is to be removed rather than that power is to be directly applied. There may not be any constipation, and yet a cathartic twice a week will do more to encourage appetite than all other means combined. Having cleared “the first passages,” tonics will be borne which would otherwise only produce fever and headache. The fluid extract of senna constitutes a convenient and efficient cathartic, and may be preceded, in cases in which there is an ordinary fullness of flesh, by a grain of calomel for a child, and a larger quantity for a greater age.

The syrup of iodide of iron, ten drops three times a day for children, intercalated with the citrate of iron and quinia, so that a child gets two grains three times a day, are favorite tonics. The employment of iodide of potassium, in doses of ten to sixty grains three times a day, often strikes some hidden cause of dyscrasia; but if continued long without interruption, it destroys the appetite. An ultimate, not a speedy, restoration is the rational aim of treatment.

JACKSONVILLE, ILL.

## IODIZED PHENOL—A NEW UTERINE ESCHAROTIC AND ALTERATIVE.

BY ROBERT BATTEY, M. D.

*Recipe No. I.*—Take of iodine, one-half ounce; crystallized carbolic acid, one ounce. Mix, and combine the two by gentle heat.

Several years ago the writer, feeling the need of a combination which should possess not only the properties of a local escharotic, but those of a local and at the same time general alterative also, devised the above formula. The subsequent use of it has proven so satisfactory, and so many and varied forms of its application in uterine disorders have suggested themselves, and especially so favorable has been the report of gynecological friends to whom the formula has been communicated, and who have largely tested its claims, it seems proper that it should now be put before the profession as a promising addition to our armamentarium.

When the iodized phenol has been applied to the cancerous uterus it has attacked the morbid growth with a good degree of energy, destroying the superficial layers very satisfactorily. The applications have not been at all painful when the sound parts have been carefully protected. Hemorrhage has been arrested very promptly, and during the continuance of the remedy has not returned. The fetor of the discharges has been most markedly diminished, and pain considerably allayed. So variable is the course of uterine cancer, it is difficult to determine the power of any remedy to retard its progress; but there is reason to believe that this method of treatment is possessed of some degree of potency in that direction.

The application to the cancerous surfaces is made upon lint or cotton, saturated with the remedy, and surrounded by a cotton tampon to protect the sound parts. A rather free serous discharge from the diseased surfaces usually occurs promptly after the contact of the iodized phenol, and combining with it would run down upon the healthy tissues if not restrained by a suitable absorbent. The application may

be repeated in four to seven days, according to the energy of the proposed treatment. When it is desired to get rid of much fungous growth, the deadened tissue is removed by the curette, and another application made without waiting for the separation of the slough. If it be wished to mitigate the escharotic, it may be diluted with glycerine to any desired degree. In cool weather, it is necessary to warm the preparation to render it liquid for convenient saturation of the dressing. By the sacrifice of a little of its energy, it may be rendered permanently liquid by the addition of a teaspoonful or two of water to the formula.

*Recipe No. II.*—Take iodized phenol, one and a half ounce; crystallized carbolic acid, one ounce; water, two drachms. Mix and make solution.

This preparation has been very fully tested by the writer in a large number of cases, and in a variety of uterine disorders; *e. g.*, chronic affections of the cervix, the cervical canal and the endometrium, uterine hypertrophy and subinvolution. It has been used both in its full strength and in various degrees of dilution with glycerine; sometimes two-thirds the above strength, sometimes one-half, one-third, and even one-fourth. The strength used has been determined, first, by the mode of application proposed; second, by the energy of the effect desired; and third, by the tolerance of the patient.

*Mode of Application.*—In some cases it has been used of full strength, and simply painted upon the cervical mucous membrane. In other cases, the whole vaginal cervix has been freely painted over, using a camel-hair pencil. More often a bit of lint cotton has been securely twisted upon the end of Budd's elastic probe, and having been saturated with the liquid, carried up to the os internum, once or twice rotated, to bring the liquid well in contact with every part of the cervical canal, and then withdrawn. At other times the elastic probe has been armed with cotton wound around it to the size of a small uterine tent, or even a large tent, the cotton being secured by thread passed several times around it, and tied with ample ends to hang out at the vulva. The cotton was then saturated with the phenol and passed into the

cervical canal, to remain for twelve or twenty-four hours, the probe having been withdrawn. In other instances, the cotton tent has been made still longer, the internal os dilated, and the saturated tent passed fully up to the fundus, there to remain for a like period of twenty-four hours. Many times these cotton tents have been allowed to remain until they were thrown off by the uterus, which has usually occurred in thirty-six to forty-eight hours. Sometimes the cervical canal has been dilated with sponge, and the interior of the uterus mopped out with the liquid on cotton secured upon the roughened end of the new aluminum probe. The latter instrument has been found to be absolutely safe against danger of leaving the cotton behind in the uterus. In a few instances, a lock of cotton has slipped from the elastic probe while in the uterine cavity, and remained for many days. It has always been thrown off by the uterus sooner or later, and no harm has resulted.

As a rule, the full strength of the formula has been used only to coat over the surfaces treated. When the saturated cotton tent has been allowed to remain, a weaker solution was used. There have been, however, a number of exceptional cases in which the full strength of the formula has been carried into the cervical canal upon a cotton tent fully saturated, and even up to the fundus itself, and allowed to remain for twenty-four hours. In these instances, it has been a little surprising to see the treatment so well borne.

Of the immediate effects of this treatment, it may be said that the pain inflicted, even by the strongest application, is for the most part very trifling, and in quite numerous instances absolutely none at all. In this respect it presents a striking contrast to the nitrate of silver. The carbolic acid, acting as a local anæsthetic, allows us to make powerful caustic applications of the iodine with little or even no pain. In most instances of its energetic application, the patient perceives, in from three to ten minutes, a decided flavor of iodine in the mouth, thus experiencing the evidence of rapid absorption of the drug by the uterus. This is further proven by the observation that a large tent even, well saturated with the black

opaque liquid, is often completely decolorized by the uterine absorption in the short space of twelve hours. In no case has any toxic effect occurred in consequence of the absorption of carbolic acid. That it is freely absorbed, along with the iodine, would seem to be proven by the fact that the odor of the acid is not to be perceived in the tent after withdrawal. The tent does not give rise to the offensive discharge which attends upon the use of sponge, nor is it itself offensive upon removal.

Of the more remote results it may be said, upon the third or fourth day, exfoliated membrane comes away in shreds or sheets, of more or less size, and in thickness corresponding somewhat to the energy of the application which has been made. Sometimes a cast of the cervical canal is seen, white in color, and of thickness so considerable as to remind one of glove kid. A discharge, more or less bloody, usually continues for one or two, sometimes three days. The applications are made ordinarily three times in the intermenstrual period, rarely oftener, sometimes but one or two each month. Under the use of the iodized phenol, excoriations and ulcerations of the os quickly heal, leucorrhœa is arrested, endometritis gradually yields and disappears; the uterine hemorrhages, which so often attend upon subinvolution, are controlled, and the uterus resumes its normal size and functions.

Whatever may have been the strength of the applications, stricture of the os and cervical canal, too often an unpleasant sequel to the use of nitrate of silver, has not resulted in any case. When applied to the cervix and cervical canal, in a caustic way, the reproduced tissue is normal and not cicatricial in character. It is believed that the very free absorption of iodine by the uterus, in this method of treatment, exerts a decidedly alterative influence over the diseased organ; and more than this, the iodine thus carried into the general circulation is highly beneficial as a constitutional remedy also. It may, therefore, be confidently asserted that iodized phenol should have a place amongst our topical applications to the diseased uterus.

## SURGICAL TREATMENT OF EPILEPSY.\*

BY GRAHAM N. FITCH, M. D.

You have heard the symptoms and medical treatment of epilepsy from another source. Its origin is recognized as centric, in the brain, the irritation transmitted thence to other parts; or eccentric, in some perhaps distant afferent nerve, from which the irritation reaches other parts through the brain by reflex action. The former is the usual origin. The seat of the cause, the source from whence the morbid action emanates, we locate doubtless correctly; but our knowledge of the character of that cause is by no means so satisfactory. It is vaguely charged to some inscrutable, indescribable malformation or functional derangement of the brain, perhaps in part hereditary. Treatment based upon such limited and speculative knowledge must be mostly empirical. I am satisfied a cause can be detected or approximated in many of those cases, the profession is usually content to treat for a name. In some cases, though few, the patient is of a sthenic diathesis, but excitable. The muscular motions are quick, energetic; pulse, perhaps, a little more full and resisting than the average of health. The epileptic countenance is wanting, unless the disease be of long standing. In such cases we near the cause if we call it excess of nervous tonicities. The overcharged nervous and muscular systems find temporary relief, an escape-valve, in a paroxysm of epilepsy; the paroxysm being usually preceded by headache and exaltation of muscular energy.

In the few of these cases I have met, I have bled at irregular intervals whenever the premonitory headache and excessive tonicity were manifest, taking not more than two or three ounces at one time. The bleedings have been followed by laxatives, combined with alteratives if the secretions were deranged, and a full sedative afterwards administered. If the patient watches himself or herself, and is carefully watched

\*From a lecture delivered at the College of Physicians and Surgeons of Indiana, January, 1877.



by the physician that these means may be used at the proper time, the paroxysms are prevented, the chain of morbid action broken, and a cure effected in a few months, with the aid of little if any other means.

In some cases in which the tonicities is evident, the sthenic habit is not so marked, but there are irregular distributions of blood. The headache occurs, and immediately preceding an epileptic paroxysm there may be momentary vertigo. The patient will then reach out to grasp any near object or person for support, or endeavor to quickly seek an easy recumbent position. In such cases, I bleed but little if any; perhaps take an ounce or less from the foot, not to deplete but to change the current of sanguineous determination. With or without these small bleedings, I insert a seton in the calf of the leg, and direct the same treatment in other respects and the same care before mentioned.

That these cases are not allied to apoplexy is evident from the character of the paroxysms, and from the fact that the treatment, especially the small bleedings, while producing or increasing a tendency to plethora, diminishes the excitable tonicity, and permanently stops the paroxysms.

There is a much more numerous class of cases having a more marked cause, very many of which the profession generally appears willing to deem idiopathic or functional, and treat accordingly; that is empirically. The cause in this class is injury of the brain, direct or through displacement of some portion of its covering. The injury may date back ten or fifteen years. At some early, perhaps almost infantile period of life, a fall or blow upon the head had been followed by more or less serious symptoms, which, however, soon disappeared, and the child's health continued good as before. At the time of the accident there may have been depression of the injured part of the cranium. The resiliency of the cranial bones at that period of life had—to a then superficial examination—apparently, but in fact not wholly, restored the depressed portion to its previous position. Or the tumefaction of the scalp, immediately after the injury, had prevented a conclusive examination, and all disagreeable symptoms having

subsided, no subsequent examination had been made. The brain accommodates itself greatly to permanent moderate pressure, especially in early life, though always during near approaching maturity, and after it, there are times when unpleasant if not dangerous symptoms occur clearly attributable to the pressure. After the accident, this accommodating disposition upon the part of the brain had permitted its development in the usual course, until the sutures and fontanelles having closed, the cranium had become unyielding. Then the relations between it and the brain began to be disturbed. The latter resented pressure, however slight, and its irritation was manifested by headaches, which were probably attributed to deranged functions of stomach or liver. Some superadded excitement occurred, and epilepsy had been developed. The present and precedent condition of the patient is inquired into by the physician. He may—too rarely, I fear—inquire whether there has been any previous injury. The lapse of time has been such the parents have forgotten the injury, and the answer is negative. Or the injury may be remembered and stated, but dating back so many years there is no suspicion upon the parents' part, nor perhaps upon that of the physician, that it occupies any such relation to the present disease as cause to effect.

In all cases of this disease, strict inquiries should be made relative to the past of the patient, and whatever the answers the cranium should be carefully examined, and if necessary the hair cut short for that purpose. Upon some portion of its arch, most frequently upon the frontal or parietal bones, its natural convexity may be found interrupted, if not by indentation, by a small plane. The disturbing force in the early injury may have been applied by the side of the sagittal suture or one of the fontanelles, and the effect be difficult of detection, because of a natural depression which often exists there. The sides of the suture and fontanelles should be compared with each other, and any difference between them noted. If it is learned from friends that an injury had formerly been received and no sign of it can be found, the patient should be directed to observe in what, if any, par-

ticular part of the head the pain commences which, in cases caused by injury, usually precedes an epileptic paroxysm. It is possible the irritation of the brain may have been the slow product of the concussion at the time of the injury without displacement of bone, and the location of the pain indicate the site of the irritation.

Having, if injury had been received, fixed its position positively by discovery of displacement, or approximately by symptoms and history of the accident, I make an incision, two or three inches in length, through the scalp and pericranium, traversing the seat of the injury and establish an issue, which I endeavor to keep suppurating two or three months. I usually have the satisfaction, after suppuration is established, of finding the paroxysms cease: out of quite a number of cases thus treated, the failures have been few. Perhaps this fact is attributable in part to my applying the treatment only to those cases to which it is properly adapted.

If the first incision manifests a disposition to heal prematurely, I make another, crossing the first or parallel with it, along the border of the seat of injury. This treatment failing after a fair trial, if there is marked depression, the trephine should be applied. It is objected to as dangerous. What important operation in surgery is not at some time? The danger, however, need not be and is not as great as usually deemed. If cautiously performed and care taken not to injure the membranes, the danger is too little to deter from the operation; especially in view of the fact that the disease when arising from depression will, in all probability, by long continued cerebral irritation, destroy the mind and ultimately fatally disorganize the brain.

Professor Gross relates four cases of trephining for this disease by himself, of which three were fatal. He tells us that in one a post mortem examination found the membranes ulcerated and the brain softened. As well might death after an operation for strangulated hernia be adduced as an argument against that operation, when an earlier resort to it would have saved life.

## Reviews.

**Studies, chiefly Clinical, in the Non-Emetic Use of Ipecacuanha,**  
with a Contribution to the Therapeutics of Cholera. By ALF. A. WOOD-  
HULL, M. D., Assistant Surgeon and Brevet Lieutenant-Colonel U. S. Army.  
Philadelphia: J. B. Lippincott & Co.

Dr. Woodhull's work may be justly styled an encyclopædia of ipecacuanha. In this little book there is compacted about all that is known or imagined, proved or conjectured, concerning the drug. The author shows himself to be a frank and conscientious narrator of what he has seen and heard, as well as an indefatigable delver after truth in the stored knowledge of the libraries. His research is really marvelous; and a painstaking reader has discovered, in this monograph of one hundred and fifty pages, nigh four hundred references to almost two hundred authors. Ancient and modern, foreign and domestic observers are quoted; and not alone from physicians is testimony taken.

Part I of the work, comprising sixty-five pages, is entitled Clinical Facts. Unfortunately many of the clinical observations are not the author's own, and some of them are accepted with scarce sufficient scrutiny. The next fifty pages (Part II) is occupied in accounting, on physiological or theoretical grounds, for the remedial action of ipecacuanha in various diseases; and the last twenty pages (Part III) is "a Speculation on Cholera," and the non-emetic use of the many-powered root in that disease.

The author holds ipecacuanha to be a substance of very extended usefulness; and to rescue it from the low estate into which it has fallen, and to vindicate it against the widely-accepted and incorrect belief that it is nothing more than a common vomit, is the object of his thesis.

Certainly no form of evacuation from the human system is so unmitigatedly disgusting as that from the stomach; and whether we call it the act of emesis, according to the Greek, or vomiting, according to the Latin, or puking, according to the Hebrew derivation of the term, yet, by any name you choose, it has still a nasty sound. The author's idea is, that emesis is not the cardinal virtue of ipecacuanha; that vomiting is not a necessary consequence of giving ipecacuanha; indeed, that it is the form of administration, or the fault of him who administers it, or of the patient, which is responsible for the gastric disturbance. A recent reviewer of Dr. Woodhull's work enthusiastically exclaims, "the theory that ipecacuanha used in any other than very small doses vomits is exploded." The *theory* may be exploded, but the *practice* of the stomach remains; and ipecacuanha still vomits just as it has always done when given in large doses, unless judicious preventive precautions are taken. Though this book is based on the non-emetic healing effects of ipecacuanha, it contains many instances where the beneficial action was clearly due to the emesis produced.

In cholera, the author believes, on theoretical grounds, that his favorite physic is likely to prove useful. In acute dysentery, chronic dysentery and diarrhoea, cholera morbus and cholera infantum, uterine and other hemorrhages, excessive perspiration, some forms of dyspepsia, vomiting of pregnancy, asthma and nervous coughs, drunkenness and delirium tremens, opium poisoning, neuralgia, intermittent fever, pneumonia, the puerperal state, acute hepatitis, antidote to venom: in all these, ipecacuanha is considered by Dr. Woodhull to be either of established or of probable efficacy; and as an enema in dysentery, and as a collyrium in conjunctivitis, the author considers it valuable. Singularly he fails to mention the treatment of dysentery by ipecac. and morphine suppository, which is often a convenient and satisfactory practice. (Ipecac. powd. ʒss.; sulph. morph. gr. iss.; cacao butter, or white soap, qs. Mix thoroughly, and divide into six suppositories; introduce one into the bowel every three to six hours.)

Nor does he allude to the wine of ipecac. in drunkard's vomiting. In the ipecacuanha treatment of acute dysentery we have had some experience, having used it almost exclusively since the appearance of Reynolds's System of Medicine in 1868, in which work this treatment is recommended. In 1868, or soon after, Dr. D. W. Yandell published in the American Practitioner a paper on this treatment, extolling it in the highest terms. In the judgment of the writer this is, above and beyond all others, the best method of curing dysentery. Unfortunately, it is difficult to get the medicine properly administered; and if close compliance with directions is not secured vomiting is apt to happen, and this, though not injurious, disgusts the patient. Even under the most favorable circumstances and most careful administration, vomiting will sometimes occur. The size of the dose (fifteen to thirty grains), the popular prejudice against the drug, and its liability to vomit, are the obstacles which have probably prevented a more general use of the ipecacuanha treatment of dysentery, for the medicine certainly deserves its ancient name of *radix antidysentericus*.

As to its value in intermittent fever, it has long been reputed to possess antiperiodic properties, and Dr. Woodhull's experiments add nothing to its former reputation. It is at best but a lame and feeble substitute for quinia, and except for soldiers, paupers and the inhabitants of prisons, it will never be much prescribed. In the nausea and emesis of drunkards, drop doses of wine of ipecac. thrice daily, or may be hourly, is an excellent remedy; and in the vomiting of pregnancy, in the same doses, wonderful effects are sometimes secured.

Time will not allow a more extended reference to the therapeutical powers of ipecacuanha, and our personal experience does not extend beyond its employment in dysentery, intermittent fever, and the vomiting in pregnancy and of drunkards. We speak now of the drug as a non-emetic.

Recumbent posture, twenty or thirty drops of tincture of opium fifteen or thirty minutes before the ipecacuanha is

given, and a mustard plaster to the stomach, together with abstinence from food and drink for three to four hours before and after giving the dose: these precautions, suggested in Reynolds's System of Medicine, we have found necessary to prevent vomiting, and they do not infallibly prevent it. Dr. Woodhull records a number of cases where none of these precautions were used, and yet no emesis followed. As a rule, the patient should not know what he is taking, lest his prejudices affect his stomach, and the medicine should be disguised by some syrup, flavor or confection.

Dr. Woodhull's work is written in beautiful English, and contains much that is valuable and interesting, and can not fail to attract the attention of the profession. L. P. Y. JR.

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**A Treatise on Hernia, with a New Process for its Radical Cure,** and Original Contributions to Operative Surgery and New Surgical Instruments. By GREENSVILLE DOWELL, M. D., Professor of Surgery in Texas Medical College, Etc. Philadelphia: D. G. Brinton. 1876. 206 pp.

In Dr. Dowell's book, thirty-four pages are devoted to a description of the varieties, causes, symptoms, course, and anatomy of hernia. His classification includes many varieties, no less than twenty-one, of abdominal hernia. A number of generally very poor illustrations are introduced, showing the anatomy and location of the various forms; also a variety of trusses for retention.

The methods that have been practiced for the radical cure of hernia are then described: by use of truss, by incision, by plugging and invagination, as practiced by Gerdy, Wutzer, Agnew and others; by ligature, as practiced by Professor John Wood; by scarification and compression, invented by Guérin; by acupuncture, recommended by Bonnet; by injection of the sack, practiced by Pancoast and others; and by subcutaneous suture, as practiced by Dr. Thomas Wood, of Cincinnati.



The author's method, which may be regarded as a modification of the one last mentioned, is the approximation of "the edges of the opening of descent" by sutures introduced subcutaneously, and retaining them until complete consolidation is effected. The instrument used by Dr. Dowell is "a double spear-pointed, semicircular needle, from four to six inches in length, with an eye near each point." The needle is armed with silver wire.

After detailing various difficulties met with in perfecting his operation, a full report is given of fifteen cases of hernia operated on by this method.

On September 12, 1876, Dr. Dowell had operated on sixty-eight cases, with sixty cures and eight failures; various other surgeons reported twenty-eight cases, with twenty cures and eight failures. No deaths had followed the operation, and serious symptoms in but a very small number of cases. Dr. Dowell may be well satisfied with this experience, and pardoned for believing "no better method for the radical cure of hernia will be ever invented." Certain of the accidents to which cases of hernia are liable are described; after which our author returns to the general subject of hernia, devoting thirty-eight pages to its various forms, their anatomy, diagnosis and treatment. All the essential facts contained in these pages being given in the earlier part of the book, this is, we think, a needless repetition.

Under the head of "Original Contributions to Operative Surgery," are noticed "causes of urinary calculi," and "the operative procedure for the relief of stone." The doctor says, "*I do not believe in lithotripsy* in the male through the urethra." (!) In lithotomy, he prefers the lateral method of operation, using Goodwin's staff. That the reader may have a sample of the doctor's style, we quote in this connection: "Dr. Goodwin's staff is fenestrated where the common staff is grooved, and his gorget is double," "and makes a bilateral incision with the center-point or probe"—(how can the probe-point make a bilateral incision?)—"but made to fit into the fenestra, after the plan of the bayonet hitch." (What is that?)

"The point of entrance is immediately at the commencement of the fenestra, and once inserted can not get out." (Why should the point of entrance want to get out?) "This is of the utmost importance, and is much better than to use Professor Post's instrument." (What is?) "For with the instrument of Dr. Post, the staff has to be withdrawn, and then the director inserted into the bladder, which is quite unnecessary with Dr. Goodwin's instruments. Dr. Post's instrument is as securely fixed, and will not permit the cutting instrument to slip out as does Goodwin's, and is, therefore, preferable to the groove director of Professor Smith." (Because it does not behave like Goodwin's, why is it better than Smith's?)

From Guy's Hospital Reports some valuable observations and several statistical tables, in relation to lithotomy, are taken.

"A new instrument for lithotomy in the male" is figured. With it, the author states, he "can perform lithotomy well and safely *blindfolded*."

Some thirty pages are given to the consideration of strictures of the urethra, various plans of treatment are discussed, and a number of cases reported. "A new uric speculum for the vagina and rectum" is described and figured; one illustration, we notice, is marked "Uric speculum, Sims's patent."

A new needle for the ligation of varicose veins is described, and cases cured by its use reported. "An original method of reducing certain dislocations of the humerus" is given; also "a new method for the reduction of the phalanges of the hand and foot." "A new diagnostic symptom of dislocation of the head of the radius, without fracture of the ulna," that certainly is not new, is described. A description of a new "arrow and bullet extractor" is given; after which comes a report of a case of osteo-fibroid tumor of inferior maxillary bone, cured by excision; also one of a case of large fibroid tumor of the thigh cured by operation.

These reports bring us to the end of this rather remarkable contribution to surgical literature. Dr. Dowell has certainly had a large surgical experience. He may be regarded as a

practical man, and as one who undoubtedly wields his knife with much more grace and precision than he does his pen. It is a misfortune that so little care was used in the composition of this book; it contains much that is of value, but often the want of precision in statement renders the meaning obscure. An example of this has been given. Here are two others from page 30, occurring in the same paragraph: "Thompson's truss has a door-spring to its lock." "Whit's truss has gun-lock springs attached to usual spring."

Notwithstanding the sharp criticisms these imperfections may evoke, if his operation for the radical cure of hernia proves as successful in the hands of others as in his own, Dr. Dowell may be well satisfied with his work. J. R. W.

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**Arsenic in Skin Diseases.** By L. DUNCAN BULKLEY, A. M., M. D. New York: D. Appleton & Co. 1876.

This monograph consists of a paper read before the section on medicine of the American Medical Association, last spring, in Philadelphia, and now published in book form. It comprises forty-five pages of condensed fact resultant from clinical observation. Dr. Bulkley shows that arsenic is not only an excellent remedy for certain skin diseases, but that when properly employed is absolutely harmless. He cites the experience of Hebra, who has often given arsenic for a period of seven years to individuals; and some of the great German's patients took as much as three hundred and fifty grains during a course, and yet he never saw a bad result from arsenic. Dr. Hunt's experience is also cited, among many others. During ten years Hunt treated in dispensary practice fifteen thousand cases of skin disease, and gave not less than *five hundred gallons* of arsenical solution during that period; and he declares it does not accumulate in the system, and is innocent of the charge often made against it of being a slow poison.

Dr. Bulkley says arsenic is particularly useful in dermatoses of arthritic, neurotic and malarial origin; and that in eczema, accompanied by severe itching, arsenic is especially efficacious. Following Dr. Bulkley's plan of giving large doses of arsenic, we have had most gratifying results in eczema and lichen, attended by periodical pruritus. We can not believe in an arthritic diathesis; and as most of the neurotic skin troubles are, to our mind, clearly malarial, we conceive the chief power of arsenic to exist in its anti-malarial property. True, the drug's usefulness is not entirely confined to this class of maladies, and wherever it may directly or indirectly promote nutrition its employment may be resorted to.

The author recommends the solutions of chloride of arsenic, arseniate of potash, arseniate of soda. Either may be given in two drop doses, largely diluted, after meals. Every second day each dose is increased two drops, until curative or physiological effects show themselves. The Asiatic pill, containing one-fifteenth of a grain of arsenic, is also mentioned; the number of pills to be gradually increased. This is Hebra's favorite way of administering arsenic.

Every practitioner should read "Arsenic in Skin Diseases." It is full of valuable information, expressed in a most concise and attractive form.

L. P. Y. JR.

**The Physician's Hand-Book for 1877.** By WILLIAM ELMER, M. D., and ALBERT D. ELMER, M. D. New York: W. A. Townsend.

Books of this kind are so numerous now that it is difficult to say which is the best, each one containing some information that others do not; but this one recommends itself as meeting every requirement. The practitioner will find it a valuable companion as a visiting record; as a ready reference to diseases, their prominent symptoms and treatment; as a materia medica, as a reference to poisons and their antidotes.

**A Treatise on the Theory and Practice of Medicine.** By JOHN SYER BRISTOWE, M. D., London, F. R. C. P., Etc. Edited with Notes by JAMES H. HUTCHINSON, M. D., Physician to the Pennsylvania Hospital, to the Children's Hospital, Etc. Philadelphia: Henry C. Lea.

The American editor, in his preface, observes that "he knows of no other work in which the author has been equally successful in bringing within the compass of a single volume the description of so large a number of diseases—some of which are not always included in works on Practice, as, for instance, diseases peculiar to women, and of the skin—and in doing this in a manner so advantageous to the student."

This is, indeed, high praise from one well qualified to judge, and we believe it is just. While the part in Bristowe's Theory and Practice devoted to diseases of the skin is excellent, and moderately complete, the diseases of women have barely eight pages allotted them. Among the very few of these latter maladies considered, are cystic tumors of the ovaries, and, marvelous to relate, these are the only ones the therapeia of which is given. That therapeia, brief as Dr. Bristowe makes it, might be still more condensed, Send for Spencer Wells. Now any one who buys the book thinking he will have any valuable knowledge upon diseases of women given him, will be greatly disappointed.

But any one who wants a good, clear, condensed work upon Practice, quite up with the most recent views in pathology, will find this a most valuable work. The additions made by Dr. Hutchinson are appropriate and useful, and so well done that we wish there were more of them.

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**Contributions to Reparative Surgery.** By GURDON BUCK, M. D. New York: D. Appleton & Co. 8vo. 237 pp.

This volume is devoted almost entirely to plastic surgery of the face, giving the writer's own experience. There are eighty-six woodcut illustrations, most all of which are portrait figures executed from photographs. The first five chapters

are devoted to general observations on transplantation of the skin, treatment of raw surfaces left to heal by granulation, sutures and their management, and modes of operation.

The book is divided into the three following classes: First, loss of parts involving the face, resulting from destructive disease or injury; second, congenital defects from arrest or excess of development; third, cicatricial contractions following burns. Many of the operations for injury and loss of substance, as from the nose, cheek, eyelids, etc., are very ingenious, and valuable to one who may have a perplexing case requiring plastic surgery of the face; for here are cases illustrated and described, which will throw light on most of the varieties.

Among the third class presented is a case of congenital hypertrophy of the tongue in a little girl. This condition occurs oftener in the female than in the male. Why it is so is not known; but it seems to confirm the theory that as a rule woman is blessed with more tongue than man. In the latter part of the book several cases of cicatricial contraction due to burns of the neck and other parts of the body are treated.

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**Ophthalmic and Otic Memoranda.** By D. B. ST. JOHN ROOSA, M. D., and EDWARD T. ELY, M. D. New York: William Wood & Co. 1876.

This little book is intended by the authors to serve as a sort of dictionary, not to be used to acquire a primary knowledge of either of these sciences. The book we should think would be most valuable to the student attending lectures, for he can carry it in his pocket and refer to it frequently. For the practitioner we do not see that it would be of much use, for the subjects treated of can be referred to almost as readily in the larger works, with fuller and more satisfactory information, with the exception of the anatomical portion, however, which will be found more complete in it than in Soelberg Wells, and some other large works.

## **Clinic of the Month.**

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CASE OF STERILITY.—Dr. William Goodell reported to the Philadelphia Obstetrical Society—*American Journal of Obstetrics*, January, 1877—a case of sterility, that points a moral which really is often presented in practice:

A strong, healthy, and well-to-do Irishwoman, about thirty years old, sought my professional advice something over a year ago. She complained of dysmenorrhœa and of some leucorrhœa; but it was mainly on account of her sterility that she consulted me. She had been married for nine years without conceiving, and her and her husband's desire for offspring was intense.

I found the usual conical cervix of a nullipara, associated with a minor degree of antelexion, and with some stenosis. Placing her under ether, I forcibly straightened out and stretched open the cervical canal with my modification of Ellinger's dilator. This operation, followed by topical treatment, gave absolute relief for some five months. But she did not, however, conceive, and at the end of that time began to have a slight return of pain at her monthly flux. At her earnest request I again etherized her, and forcibly dilated the cervical canal; but still she remained barren. She now so begged and so urged me to perform any operation which would be likely to make her fruitful, that, although the amount of dysmenorrhœa was really not sufficient to warrant such measures, I was debating in my mind whether to slit open the cervix or to amputate it, or to let it alone.

While yet undecided what course to pursue, I received another visit from her, which put matters in a different shape. She told me that her husband had picked up one of those vile



pamphlets, which, while pandering to our lowest instincts, serve as advertisements to unscrupulous quacks. From reading its contents, he was led to think that the fault lay on his side and not on his wife's. He accordingly, without her knowledge, called on the authors of this pamphlet—for they generally hunt in couples—and stated his case to them. They assured him that "his seed was dead," but offered to restore it to life for the consideration of seventy-five dollars. This sum he paid down at once, and began a course of treatment, consisting of a few shocks from a battery, and of more medicine. After two weeks had elapsed they told him that his case was one needing a far more active treatment than they had at first supposed, and demanded seventy-five dollars more. Before paying this additional sum, he made a clean breast to his wife of what he had done; and she at once sent him to me for advice in the matter. He appeared to be a little older than his wife, was in splendid health, and had since puberty contracted no other disease than a gonorrhœa, which, fifteen years ago, kept him in bed for some days from inflamed testicles. Notwithstanding this, he confessed to vigorous virile powers, and had no complaint to make against his wife on that score. He was an unusually intelligent man, and I was determined to rescue him out of the hands of these harpies. Having shaken his confidence in them by explaining their old trick of showing under the microscope lively vinegar-eels as a proof of virility restored by their treatment, I sent him with a note to a medical friend, who is acknowledged to be one of our best microscopists. This gentleman obtained a fresh specimen of my patient's semen, and, after examining it with the utmost care, failed to find a single dead or a single living spermatozoon. So amazed was my friend at this unexpected result, that, fearing some source of error on his part, he obtained a second specimen of this semen, and submitted it to other experts besides himself. Yet not a trace of a spermatozoon, living or dead, could be discovered.

This extremely interesting case leads me to think that physicians too often charge the wife with the fault of unfruitful-

ness, when it lies with the husband. As I look back on my practice I feel conscious of having made this error—an error which can be avoided only by a microscopic examination of the semen. Such a diagnostic procedure is, however, not only somewhat delicate to propose, but repulsive to make. Hence it is that the cervix has so often, as I believe, been unnecessarily incised or dilated, or otherwise maltreated. I am also led to think that, in the majority of those cases of unfruitful marriage which have been attributed to a latent gonorrhœa in the woman, it is reasonable to attribute the sterility less to an alleged secondary infection of the wife's organs, than to the primary disorder of the husband's. In the interest of science, then, as well as of humanity, all unfruitful marriages demand such a means of diagnosis as shall attach the blame where it belongs, and without doubt.

PHIMOSIS, INCOÖRDINATION OF MOVEMENTS, WITH LOSS OF EQUILIBRATING POWER—CIRCUMCISION AND RECOVERY.—In the *Boston Medical and Surgical Journal*, January 18, 1877, E. P. Hurd, M. D., relates an interesting case, with the above conditions. The patient, a bright lad of seven years, had been in poor health for several months; he was nervous, pale, took little notice of anything, and said but little. The intellect, however, was not disturbed. Locomotor ataxia was a marked symptom. He could not walk across the room without struggling and pitching headlong, and could not coördinate sufficiently to sit up or feed himself. He responded to questions in monosyllables, his speech not being very distinct. Pupils were widely dilated, with some squinting, due, as was supposed, to paresis of the third nerve. There was dullness of hearing, no fever, pulse normal, no complaint of pain, but hyperæsthesia of the general surface. A few days after first seeing him he had a restless night, frequently screaming, and on the following night had a severe epileptiform fit. There was no constipation or difficulty of micturition. He was treated with nervous sedatives for more than a week, which partially quieted him, but did not improve his general condition.

One day when the child was naked in its mother's arms, phimosis was discovered; the prepuce was greatly elongated, strangulating the glans, and the urinary punctum was minute. Circumcision was performed, and improvement progressed steadily from this time until the child was well.

The doctor remarks that this must be considered a case in point where the ataxia was purely functional, owing to irritation of the glandular branches of the dorsalis penis nerve, the speedy relief following circumcision demonstrating this; and that had the phimosis not been discovered it is natural to suppose that general paralysis, with dementia, would have been the ultimate result.

ON THE CURATIVE INFLUENCE OF AN EXCLUSIVE MILK DIET.  
Dr. A. Scott Donkin, in the *Lancet* of December 30, 1876, advances the following views on this subject:

Two points are discussed, first, whether it is best to administer skimmed or unskimmed milk; second, the mode of administering the milk. Unskimmed milk is preferred by some because on account of the cream it contains it is less likely to produce constipation, but this can be obviated by a little judicious management. Moreover, constipation is a sure sign that the treatment is agreeing with the patient; whereas, diarrhœa is a very untoward indication, more apt to be induced by unskimmed than by skimmed milk; hence, in the treatment of the diarrhœa of typhoid fever and dysentery, the latter is very superior to the former. Dr. Donkin's objections to unskimmed milk as compared with skimmed are, first, on account of the cream in the former, it very often disagrees with the patient when taken as an exclusive diet to the extent of from three to six pints required for the nutrition of the adult in Bright's disease. In the next place, unskimmed milk is not so powerfully diuretic as skim-milk, because it contains less casein or albuminate than the latter, on which the extraordinary diuretic property of milk in renal dropsy undoubtedly depends. The cream of unskimmed milk is highly pernicious in chronic nephritis when fatty degeneration of the glandular

epithelium has begun, for frequently the albuminuria and dropsy increase when unskimmed has been substituted for skimmed milk.

It is necessary to begin the milk with doses of a wineglassful, or even less, and gradually to increase the quantity until three to six pints are taken daily. A patient should not be permitted to drink as much milk as he can and whenever he pleases, but the most rigorous rules as to quantity and intervals of administration should be observed.

The diuretic action of skim-milk, in renal dropsy, is generally remarkable, even after all ordinary diuretics have failed. Genuine milk has a specific gravity of 1030 to 1035; skim-milk, proportionally richer in casein and salts, of 1035 to 1040. The continuous digestion and absorption into the blood exclusively of a food so rich in albuminates, and of so high a density into a blood rendered hydræmic, and with a serum reduced in specific gravity to 1020 or 1016 from the loss of albumen, immediately puts into action the force of osmosis. A rapid influx of blood from without into the enriched blood is induced and continued; the result being vascular plethora, which is relieved by profuse diuresis; this in its turn washes out the uriniferous tubules previously choked up and distended by tube casts and diseased epithelium. The pressure of the swollen tubules is thus removed from the renal capillary system, a normal circulation through them becomes reëstablished, the distention of the malpighian tufts is relieved, the albuminuria and dropsy disappear *pari passu*; and, finally, a healthy nutrition is restored to the kidneys, where we have to deal with the inflammatory form of Bright's disease.

NITRIC ACID FOR HOARSENESS.—Dr. W. Handsel Griffiths says that a few drops of nitric acid in a glass of sweetened water, a couple of times daily, will be found an excellent remedy for the hoarseness of singers. One of the largest fees ever received by him—so he says—was for this prescription. (New Remedies.)

**LACTIC ACID AS A HYPNOTIC.**—On the 15th of March, Herr E. Mendel read a paper before the Medical Society of Berlin, on the Hypnotic Properties of Lactic Acid, and referring to the observations of Preyer and Lothar Meyer on this subject, he said that its effects, when administered by the mouth, either pure or in the form of the lactate of soda, were uncertain, but he had found very good results from its use in enemata in a large number of cases. The dose of lactic acid which he recommended was five to twenty grammes (75 to 300 grains), mixed with an equal quantity of lactate of soda. The use of lactic acid was specially recommended: First. In cases of insomnia in the course of debilitating diseases, or during convalescence from them, after hemorrhages, etc. Second. As a calmative in the excitement of the insane. Third. As a remedy in certain psychoses, in regard to which its precise indications must yet be determined.

In a discussion which followed at the next meeting, Herr Senator said that he had used lactic acid, either in divided doses, two grammes (155 grains) being given in the course of a day; or in single doses of five to ten grammes in gaseous water, or as lemonade. With the first-named mode of administration no sleepiness was observed. On the other hand, a large single dose produced pure sleepiness, although lactic acid could not be compared, as regards strength or duration of action, with morphia or with chloral hydrate. There was, however, a troublesome after-effect, which had not been noticed by Herr Mendel or by Lothar Meyer—the occurrence of rheumatic pains; these he had observed twice, once in a phthisical patient, the other time in a man who had frequent attacks of muscular rheumatism. Rheumatic pains had also been observed in giving lactic acid with other objects, such as the treatment of diabetes, etc. (New Remedies.)

## *Notes and Queries.*

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VERY LARGE BILIARY CALCULUS PASSED BY THE RECTUM. We are indebted to Doctors W. F. and J. Reilly, of Sardinia, Decatur county, Indiana, for an extraordinarily large biliary calculus passed by one of their patients. The history briefly given is this: Mrs. —, sixty-five years of age, mother of eight children, was attacked last spring with diarrhoea; the abdomen was swollen and tender, especially in the right hypochondrium; she had paroxysmal pain in the abdomen, and it was attended with bilious vomiting. Then followed what seemed to be typhoid fever, lasting six weeks. Three weeks subsequent to recovery she passed from the bowel the calculus, after four hours of severe suffering. She is confident she had previously evacuated similar, though much smaller, formations. Her health is now much better than it has been for some years.

We give two illustrations, the first showing the size and shape of the calculus, the other the appearance upon transverse section.



The measurements of the stone, which it will be observed is an ellipsoid, are, long diameter one and five-eighths inches, short diameter one inch. Its weight is one hundred and ninety grains. The nucleus, according to my friend, Dr. Henry Jameson, is of dark biliary resin, while the mass is of

cholesterine, extending in regular radii from that nucleus. Generally the nucleus of these formations is of solidified bile, sometimes a blood-clot, at others a dead entozoon, and again of globules of mercury in those who have been treated for constitutional syphilis.

Leichtenstern, Ziemssen's *Cyclopædia*, Vol. VII, page 574, observes: "It is a fact, proved by many observations, that even large gall-stones can pass gradually through the distensible bile-ducts, and in this way reach the intestine. Abercrombie found, in a fatal case of lodgment of a gall-stone in the small intestine—the stone measured four inches in longitudinal and three and a half in transverse circumference—the ductus choledochus communis so enlarged that it would admit a finger."

Luton, *Nouveau Dictionnaire de Médecine et de Chirurgie Pratiques*, Vol. V, page 75, remarks that when these calculi, evacuated by the bowels, are as large as a walnut or a pullet's egg, it is difficult to admit that they have passed through the ductus choledochus. It is more probable, as has been several times positively ascertained, that by a fistulous communication between the gall-bladder and the duodenum or the transverse colon, the expulsion has been effected. Doubtless this occurred in the case of Friedler, where the patient discharged by the anus two calculi of cholesterine, their joint weight being four hundred and forty-five grains. In the example of a calculus, two centimeters in length and a centimeter and a half in diameter, passed by the anus, observed by Herard, this explanation was admitted by Cruveilhier.

Frerichs, *Clinical Treatise on Diseases of the Liver*, Vol. II, page 499, remarks: "The size of gall-stones varies from that of a millet-seed to that of a hen's egg. J. F. Meckel has described a solitary calculus measuring five Paris inches in length, and four inches in circumference. I have repeatedly met with concretions, from two to two and a half Paris inches long, and one inch thick."\*

\* One Paris inch contains twelve Paris lines; one English inch contains only 11.25 Paris lines.



FETAL MONSTROSITY.—The following description of a monstrosity is given by Mr. R. U. Sterrett, a student of the College of Physicians and Surgeons of Indiana:

The accompanying woodcut is designed to illustrate a curious fetal monstrosity which came under my observation some time ago, at the College of Physicians and Surgeons in this city. A brief description may be of some interest. The head is very much larger in proportion than the remainder of the body, measuring nearly fifteen inches in circumference; this extraordinary size being due to hydrocephalus. The bones of



the skull can very readily be displaced and moved about beneath the scalp, there being considerable space between their borders on account of the great distention of the scalp, caused by the fluid contained within the cranial cavity. In the anterior fontanelle, projecting forward and downward between the two divisions of the frontal bone, there is a thin, cartilaginous or osseous structure, elliptical in its outline and distinctly separated from the frontal and parietal bones (*a*). The face presents a horrible, irregular opening, on account of the double harelip and cleft palate. The mouth extends nearly to the

ear, on the right side; and the entire opening, including the anterior nares, presents a ragged, uneven border. Projecting from the anterior nares is a round, hard tubercle (*b*), which is continuous behind with the septum of the nose; or rather which seems to be a union of a part of the palatal process of the superior maxillary and the vomer (*c*). The palatal portion of the superior maxilla is divided, and presents two roughened masses (*d*), which project from either side into the cavity of the mouth. Between these two masses, the portion of bone alluded to above (*c*), passes backward in a curved direction, and bears nearly the same relation to the two portions of the superior maxilla, that the keystone does to the arch. The right ear is perfect, but just in front of it there are three small nipple-like eminences, varying in size from an eighth to a quarter of an inch in diameter. The left ear is imperfect: the meatus externus being imperforate, and of the pinna only the tragus and lobule are developed; the upper and greater portion is entirely absent. The remaining portions of the body, the trunk and extremities, are perfectly formed.

DR. WRIGHT ON CEPHALIC VERSION.—The subjoined letter, though not intended for publication, is so practical that recognizing its benefit to us, we feel we ought to let our readers participate the benefit:

*My Dear Dr. Parvin:* Your case of shoulder presentation, published in the last number of the Practitioner, I have read with much interest. It has reminded me of several points not clearly understood alike by members of the profession; and I hope you will allow me to state them briefly for your individual consideration, when your mind is comparatively free from more important duties.

Writers and speakers are apt to allude to podalic version as of easy accomplishment. So it is oftentimes. But there are cases which subject to severe test the best skill of the experienced practitioner. The same remarks will be allowed as applicable to cephalic version; and upon this point I shall offer a few words of explanation presently.

Practically, it may be a question of no importance whether the presence of the shoulder at the superior strait is to be attributed to over-distention of the uterus by liquor amnii, and too extensive a range of the fetus; to obliquity of the uterus, or deformity of the pelvis. An opinion upon either of these conditions, as a cause of malpresentation, would be at best mere conjecture. How common is an excess of liquor amnii, and are departures from normal structures of the pelvis; and how seldom do we encounter in these cases malpresentations! Again, some women are annoyed by the constant motion of the fetus, while others feel only slight tappings at long intervals. Still the former are no more subject to difficult labor of any kind than the latter. In using the term *practically*, I mean that a shoulder presentation at term can not be successfully delivered as a shoulder presentation. If a narrow or deformed pelvis should add to the complication, the question how shall delivery be effected, must stand out by itself. My attention has been more directed to the inquiry, when produced rather than how produced. Cases of long standing, congenital as it were, I have imagined to be more difficult of management than those more recent, accidental. In the former, after the shoulder has been moved out of the way, there has been a strong tendency to return, requiring pressure to be continued until the vertex under uterine action has fairly taken its position at the superior strait. In the latter, the change of presentation is not only more easily effected, but does not require repetition.

Guided by experience, I should say that no decided effort should be made at cephalic version until after the complete dilatation of the os uteri. Command of the entire shoulder is needed, and this can not be secured while the partially dilated os interferes with the free use of the hand.

Again, sufficient force, direct and continuous, can not be applied to the shoulder while the membranes are unruptured and tense. If their integrity can be preserved until the second stage of labor has become somewhat advanced, so much the better; but their rupture must precede the force that is to be applied to get the shoulder out of the way.

A single unsuccessful case has too often led to unchangeable prejudice against the means used for the correction of evil. This has been especially so with some of my friends who have availed themselves of the knee and breast position as an aid to podalic version. Their want of success, I am sure, was to be attributed to something extraneous to position.

Your case suggests the propriety of placing patients with shoulder presentation in the genu-pectoral position at an early period in labor, not so much in anticipation of "spontaneous evolution," as to secure gravitation of the breech in aid of outside pressure.

M. B. WRIGHT.

*Cincinnati.*

DELIVERY OF A FETUS WITHOUT RUPTURE OF THE MEMBRANES.—Dr. T. Chesnut, Lafayette, Ind., writes us that recently he was called to attend a case of labor, and on entering the room of the patient, and making an examination, found that the uterus had expelled a well-developed fetus completely enveloped in its membranes. After a careful inspection no perforation of the membranes was found, so he speedily relieved the child of its investments and saw that the cord still pulsated; soon afterward the child gave manifestations of independent life by a distinct cry followed by respiration. Giving attention to the woman now, he found the uterus large and flaccid from imperfect contraction, but as there was no hemorrhage he left it to contract of its own accord, which it did in a short time without the aid of ergot or manual interference. The interesting features in this case consist in two well marked departures from the ordinary course of nature: First, the unusual length of time that the placental circulation was maintained after expulsion of the child, at least twenty-five minutes. This delay of the uterus in contracting was conservative, for had it contracted within five or ten minutes after delivery, as is usual, the placental circulation would have been cut off, and the child not being in a condition to carry on respiration would have died. Second, the strange freak of nature in forcing the child from the uterus without first disrobing it of the intra-uterine investments.

A COINCIDENCE.—Thirty-two years ago this winter, Doctors Austin Flint and Graham N. Fitch were colleagues in Rush Medical College. In the spring of 1845 they parted; the former not lecturing again in "Rush," the latter continuing his connection with it a few years longer, and their paths have never again met. In this number of the American Practitioner these able and eminent men meet again as colleagues, but address a much larger number than the small class that in 1844-5 assembled in Chicago.

Dr. Fitch has been constantly engaged in his profession since 1830, with the exception of about ten years in public life. Those ten years—and the statement is made for the benefit of any medical gentleman who is desirous of political place—he now wishes had been given his profession. This testimony certainly is very strong, when we are reminded that he was not only a member of the state legislature, and of the convention for the revision of the constitution of the state, but also a United States senator, and a political leader of national influence and fame.

A REMARKABLE DERMOID CYST.\*—On the 12th of January, whilst making a post mortem examination upon a woman aged fifty-two years, who had died from starvation (owing to stricture of the œsophagus), there was found a tumor occupying the situation, and about the size of a fully distended bladder. Upon opening the abdominal walls, a large cyst was discovered involving the left ovary, whilst the fimbriated extremity of the fallopian tube was expanded upon the upper surface, and was of a dark red color. Otherwise the walls of this cyst were smooth, laminated and polished on both surfaces. Upon opening the cyst, a large quantity of yellowish fluid, at first thought to be pus, poured out, but upon cooling it became hard, and was about the consistence of melted tallow. In the center of this fatty mass were some lumps of the same material, which was as hard as stearine, besides a

\* We are indebted to Dr. William B. Fletcher, one of the faculty of the Indiana Medical College, for this report.

mass of hair as large as the fist, many of the hairs measuring twenty-two inches. At the bottom of the cyst nearest the ovary was found a mass of dermal tissue about half an inch wide and two inches long, which presents, under the microscope, all the elements of true skin, and upon which was growing hair measuring eight inches long. The circulation was through a series of vessels ending in the fimbriated extremity of the fallopian tube, where they expanded like a small placenta. Tumors of this kind are mentioned by Paget, Jones, Sieveking, Wilkes, Moxon, and others. This was remarkable for the large amount of piliform matter, as well as the vast accumulation of sebaceous secretion. The specimen has been preserved in the medical museum. F.

MALE WET NURSES.—The *Gazette Obstétricale*, December 20, 1876, has, from the *Journal des Sages Femmes*, a notice of a German physician in Pomerania, who makes a specialty of producing wet nurses, that is of exciting the secretion of milk, independent of any pregnancy, in women; moreover, he does the same in men. When applied to for a nurse, he inquires of the applicant whether male or female is wanted, and, strange to say, by some families the male is preferred, they believing that thus greater vigor is given their offspring.

Shall we call it fortunate that this department of industry has been opened up to starving men? But what will women say to the invasion of what has hitherto been regarded as one of their peculiar and inalienable rights?

The late Dr. Dunglison has recorded, in his *Physiology*, one of the most interesting cases of a male wet nurse. The man—a negro in Maryland—had large, soft, well formed mammæ, rather more conical than those of the female, and projecting fully seven inches from the chest, and having perfect and large nipples. He had officiated for many years as wet nurse in the family of his mistress; and he represented that the secretion of milk was induced by applying the children intrusted to his care to the breasts during the night.